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Intentional and Unintentional Contributions to Nonspecific Preparation During Reaction Time Foreperiods

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The nonspecific preparation that follows a warning stimulus (WS) to speed responding to an impending imperative stimulus (IS) is generally viewed as a strategic, intentional process. An alternative view holds that WS acts as a conditioned stimulus that unintentionally elicits a tendency to respond at the moment of IS presentation as a result of a process of trace conditioning. These views were contrasted as explanatory frameworks for classical effects on reaction time of the duration and intertrial variability of the foreperiod, the interval between WS and IS. It is shown that the conditioning view accounts for the available data at least as well as the strategic view. In addition, the results of 3 experiments provide support for the conditioning view by showing that unintentional contributions to nonspecific preparation can be dissociated from intentional contributions.

It is generally agreed that people may enhance the speed and accuracy of subsequent actions by means of preparation. There is much less agreement, however, on the nature and mechanism of preparation. The common view is that people regulate their preparatory state in a voluntary way; that is, they preset mental structures in accordance with their intentions before a stimulus calls for action. However, recent empirical studies on shifting task set have shown remarkable limitations in the role of intention in the preparatory process (Allport, Styles, & Hsieh, 1994; Meiran, 1996; Rogers & Monsell, 1995). These studies have shown that, regardless of the duration of a preparatory interval, participants obtain a lower preparatory state with respect to a specific task set when that set differs from the task set used on the preceding trial than when it is the same as the task set used on the preceding trial. This led Allport et al. (1994) to suggest that the mental system is subject to inertia, so that the preparatory state is at least partially determined by recently performed actions.

In the present article, we extend this discussion to effects of foreperiod (FP), the interval between a warning stimulus (WS) and an imperative stimulus (IS). It has long been known that both the duration of FP and its intertrial variability have marked effects on reaction time (RT; e.g., Woodrow, 1914; Wundt, 1887; see Niemi & Näätänen, 1981, for a review). These effects are commonly attributed to the participant's state of nonspecific preparation—nonspecific because WS provides no information about the content of IS or the response required to IS, but merely about its moment of presentation. According to the strategic view, the state of nonspecific preparation reflects an intentionally driven preparatory process that is guided by expectancies about the moment of IS presentation. By contrast, according to the conditioning view recently put forward by Los, Knol, and Boers (2001; see also Los, 1996), the state of nonspecific preparation corresponds to a conditioned response unintentionally elicited by WS. In this article, we show that the conditioning view accounts for several effects of FP at least as well as the strategic view. In addition, we provide empirical support for the conditioning view by dissociating intentional and unintentional contributions to the state of nonspecific preparation.

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Basic Design and Phenomena

The starting point of our discussion is a set of well-established phenomena that can be observed in variations on a basic design. To understand this design, suppose that four levels of FP (e.g., 0.5, 1.0, 1.5, and 2.0 s) are presented in both pure and mixed blocks of trials. When presented in a pure block, only one level of FP occurs across the trials of a block, and so four pure blocks are required to present the four different levels of FP. When presented in a mixed block, all levels of FP occur (randomly) across the trials of a block. For the moment, we consider only a uniform distribution of FPs in mixed blocks (i.e., when each level of FP has an equal probability of occurrence on each trial); we return to other distributions of FP in the General Discussion section. Furthermore, we introduce the concepts *critical moment* and *imperative moment* (cf. Los et al., 2001). A critical moment is a possible time of IS presentation relative to WS in a given block of trials. The imperative moment is the actual time of IS presentation on a given trial. Thus, in our example, a mixed block has four critical moments at 0.5, 1.0, 1.5, and 2.0 s from WS, whereas, on any specific trial, only one of these moments is imperative. In a pure block, only one moment is critical, and it coincides with the imperative moment on each trial.

Two effects are invariably found in studies with this design. First, the effect of FP on mean RT depends on block type. In pure blocks, mean RT increases linearly with FP[1] (e.g., Bertelson & Tisseyre, 1968; Elliot, 1970; Holender & Bertelson, 1975; Simon & Slaviero, 1975), whereas, in mixed blocks, mean RT is longest for the shortest FP and decreases as a negatively accelerating function of FP (Bertelson & Tisseyre, 1968; Elliot, 1970; Requin & Granjon, 1969; Requin, Granjon, Durup, & Reynard, 1973). Second, there are sequential effects in mixed blocks, such that the effect of FP on a given trial depends on the duration of the FP that occurred on the preceding trial. Specifically, longer RTs have been reported for a given FP when it is preceded by a longer FP than when it is preceded by an equally long or shorter FP (e.g., Baumeister & Joubert, 1969; Drazin, 1961; Possamaï, Granjon, Reynard, & Requin, 1975). That is, sequential effects of FP are asymmetric in that long FPs prolong RT for subsequent shorter FPs, whereas the converse is not true.

We emphasize that these two effects are not independent. Asymmetric sequential effects in mixed blocks strongly, if not totally, determine the effect of FP on mean RT. The longer an FP, the lower the probability that an even longer FP occurred on the preceding trial. Hence, as FP lengthens, responding is, on average, less subject to the slowing influence of the preceding trial.

The Strategic View of Nonspecific Preparation

The strategic view of preparation goes back to the classical experimental work by Woodrow (1914; see Niemi & Näätänen, 1981, for a review). The underlying assumption of this view is that an optimal preparatory state is rapidly reached but can be maintained for only a brief period (e.g., Alegria, 1975; Gottsdanker, 1975; Näätänen, 1972). Thus, participants can bring about peaks of preparation and respond more quickly as these peaks have more temporal overlap with the imperative moment. The degree of overlap is in turn affected by two factors, *time uncertainty* and *expectancy*.

Time uncertainty refers to the participant's inability to estimate perfectly the moment of presentation of an expected event. Klemmer (1957) showed that, in a synchronization task in which participants were asked to synchronize their response with the onset of IS, the variance in response latency increased as a function of FP, indicating that a participant's time uncertainty increases as FP increases. In the context of RT tasks, this increase in time uncertainty is assumed to reduce the participant's preparatory state at the imperative moment, which in turn prolongs RT (Gottsdanker, 1970; Klemmer, 1956, 1957; Näätänen & Merisalo, 1977; Niemi & Näätänen, 1981). In pure blocks, time uncertainty is the only factor that determines the variation in the preparatory state, which explains the observed RT increase as a function of FP. In mixed blocks, however, time uncertainty is considered to be dominated by the expectancy of the participant as to which of several critical moments is going to be imperative on a given trial. Initially, it was assumed that expectancy strongly correlates with the objective conditional probability of IS presentation, which is low at the start of a trial and increases as time goes by without IS presentation. The corresponding growth of expectancy is assumed to enhance the participant's preparatory state, which in turn speeds responding (Baumeister & Joubert, 1969; Drazin, 1961; Niemi & Näätänen, 1981; Requin & Granjon, 1969; Stilitz, 1972). Thus, time uncertainty along with expectancy in terms of the conditional probability of IS presentation provides an adequate explanation for FP effects on mean RT in both pure and mixed blocks, and consequently these concepts have found general acceptance in the literature (e.g., Luce, 1986; Sperling & Dosher, 1986).

Unfortunately, the traditional concept of expectancy has the fundamental problem that it is oblivious to sequential effects. Therefore, alternative ways to conceive of expectancy proved necessary. One proposal (Niemi & Näätänen, 1981; Requin, Brener, & Ring, 1991) is that, at the start of a trial in a mixed block, participants aim their preparation at the critical moment that was imperative on the preceding trial (i.e., they expect an FP repetition) and prepare again for a later critical moment if IS has not occurred by that time. Consequently, when the imperative moment occurs earlier than on the preceding trial, RT will be long, whereas when the imperative moment occurs later than on the preceding trial, a long RT may be prevented by means of repreparation for a later critical moment.

Although this proposal accounts for both sequential and mean effects of FP, it has lost the parsimony of the initial concept of expectancy in terms of the conditional probability of IS presentation. In particular, its assumption that participants use a strategy of aiming their preparation at the critical moment that was imperative on the preceding trial is suspect for at least two reasons. First, it begs the question of why participants do not opt for the more successful strategy of preparing on each trial for the first critical moment and initiate the repreparation cycle from there. Second, there is substantial evidence that even with two equiprobable alternatives, people tend to expect an alternation rather than a repetition of events, as illustrated by the gambler's fallacy (Jarvik, 1951; Soetens, 1998; Soetens, Boer, & Hueting, 1985; Wagenaar, 1972). Despite these problems, the intentional nature of nonspecific preparation has never been seriously challenged.

The Conditioning View of Nonspecific Preparation

Los et al. (2001) highlighted the similarity between the FP design under present examination and designs used to examine trace conditioning in animals. Trace conditioning is a form of classical or operant conditioning defined by a blank interstimulus interval between a conditioned stimulus and an unconditioned stimulus. It is well established that after some acquisition training, a conditioned response develops that is time locked to the conditioned stimulus and obtains its peak at or about the moment the unconditioned stimulus is presented (e.g., Gallistel & Gibbon, 2000; Grossberg & Merrill, 1992; Machado, 1997; Roberts, 1998). Los et al. argued that nonspecific preparation may rely on the same principles as trace conditioning, because critical moments in the FP design are time locked to WS.

This view relies on four assumptions (cf. Los, 1996; Los et al., in press). First, corresponding to each critical moment there is a state of conditioning, the adjustment of which is governed by

[1] Obviously, this linearity holds only for intermediate FPs. On the one hand, the linearity must break down somewhere for long FPs, when ultimately an asymptotic RT value is reached reflecting a completely unprepared state (e.g., Warrick, Kibler, Topmiller, & Bates, 1964; Woodrow, 1914). On the other hand, the FP–RT function becomes U shaped when FP is extended down to 0 s, when the lowest point is found for an FP somewhere between 100 ms and 300 ms (Bertelson, 1967; Sperling & Dosher, 1986).

learning rules of trace conditioning (specified subsequently). The state of conditioning implicates an increase and decay of response-related activation as a critical moment is bypassed in time. This conditioned response is preparatory in nature, because an increase in response-related activation does not lead to an overt response as long as the motor-action limit is not exceeded (cf. Näätänen, 1971; Näätänen & Merisalo, 1977). Second, the conditioned response takes more time to build up and decay and its asymptotic value is lower when its corresponding critical moment is more remote from the warning signal. Third, on any trial, the strength of the conditioned response corresponding to a critical moment is reinforced (i.e., increased toward its asymptote) if and only if that critical moment coincides with the imperative moment. Fourth, and most characteristic for the model, on any trial the strength of the conditioned response is extinguished (i.e., driven away from its asymptote) if and only if its corresponding critical moment occurs before the imperative moment, whereas it is left unaffected if its corresponding critical moment occurs later than the imperative moment. The rationale underlying the latter assumption is that quickly rising conditioned activation during FP should be suppressed to prevent premature responding, whereas this need is cancelled by the event of IS presentation.

When it is furthermore assumed that RT is inversely related to the strength of the conditioned response at the imperative moment, a model based on the preceding four assumptions readily accounts for the effects of FP in pure and mixed blocks. This is illustrated in Figure 1. In pure blocks, there is only one critical moment, for which the corresponding strength of the conditioned response approaches its asymptote after some trials as a result of repeated reinforcement (Assumption 3). Because the asymptote is lower as a critical moment is more remote from WS (Assumption 2; Figure 1A), there is a decrease in the preparatory state and a corresponding increase in RT as FP lengthens. In mixed blocks, the rules for reinforcement and extinction constitute a different picture. Suppose that at the start of trial *n*, there is an equal state of conditioning corresponding to each critical moment (Figure 1B). Now suppose that, on trial *n*, the earliest critical moment of 0.5 s is imperative. Then, after trial *n*, the state of conditioning corresponding to this critical moment is increased as a result of reinforcement (Assumption 3), whereas the state of conditioning corresponding to the other critical moments is left unchanged because they were not bypassed during FP (Assumption 4; Figure 1C). Now, suppose that on trial $n + 1$ the latest critical moment of 2.0 s is imperative. Then, after trial $n + 1$, the state of conditioning corresponding to this critical moment is increased, whereas the state of conditioning corresponding to the other critical moments is decreased because they were bypassed during FP (Assumption 4; Figure 1D). Across the trials of a mixed block, these dynamics culminate in an average state of conditioning, depicted in Figure 1E. The state of conditioning corresponding to the earliest critical moment is lowest, because it receives extinction on 75% of the trials (i.e., whenever another critical moment is imperative). It is progressively higher for subsequent critical moments, which are increasingly less bypassed during FP in the course of a block (i.e., on 50%, 25%, and 0% of the trials for FPs of 1.0, 1.5, and 2.0 s, respectively).

We emphasize that the state of conditioning depicted in Figure 1E is merely an average that is not necessarily representative of *any single trial*. Especially the state of conditioning corresponding to early critical moments may vary strongly from trial to trial,

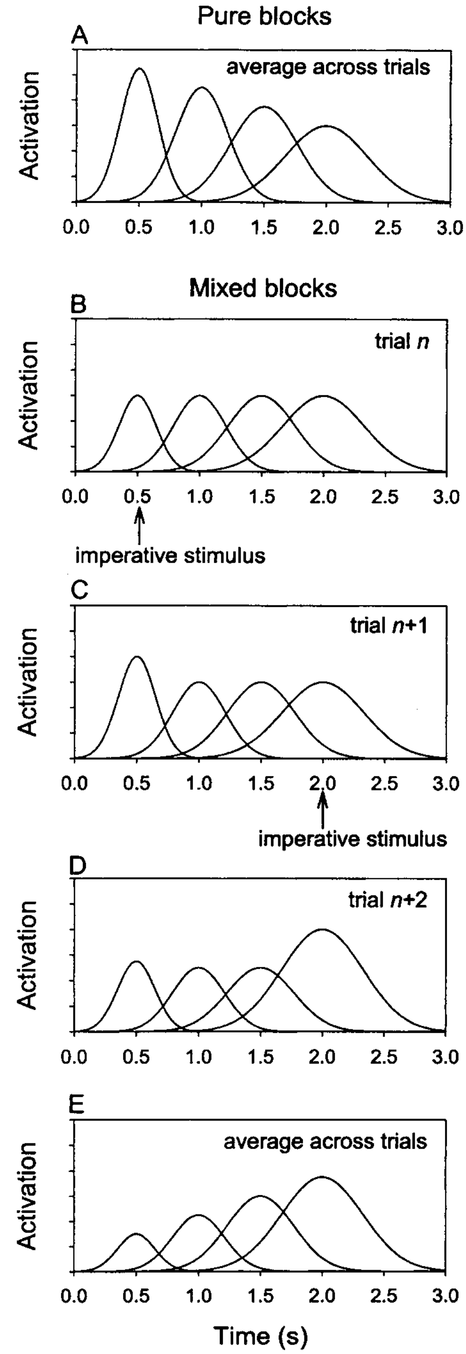

Figure 1. Theoretical states of conditioned activation corresponding to the critical moments in pure and mixed blocks according to the conditioning view of preparation. A: In pure blocks, the conditioned activation has a lower peak and is more smeared over the time scale as its corresponding critical moment is more remote from the warning signal. B–D: Adjustment of the conditioned activation across three subsequent trials in mixed blocks. For the critical moment that coincides with the imperative moment (i.e., the moment used for the presentation of the imperative stimulus), the corresponding state of conditioning is reinforced (Panels B–D). For critical moments that occur later than the imperative moment, the corresponding state of conditioning is left unchanged (Panels B and C). For critical moments that occur earlier than the imperative moment, the corresponding states of conditioning are subject to extinction (Panels C and D). E: The state of conditioning in mixed blocks when averaged across trials.

depending on the duration of the preceding FP. Indeed, dynamic adjustments of conditioned activation are at the core of the conditioning view. These adjustments account directly for the occurrence of asymmetric sequential effects and only indirectly for effects on mean RT. Thus, the conditioning view meets the objective that a good explanation for FP effects on RT should, first and foremost, account for sequential effects.

The Nature of Nonspecific Preparation

At the start of this article, we rather loosely distinguished the strategic view and the conditioning view on the basis of the role of intention. In this section, we make this distinction more precise.

The critical sense in which we think nonspecific preparation is intentional according to the strategic view but unintentional according to the conditioning view is with respect to the within-trial development of preparation at critical moments. Within the bounds of the strategic view, several researchers have argued in favor of the strategic nature of this preparation (e.g., Alegria, 1975; Gottsdanker, 1975; Näätänen, 1971), as vividly expressed by Gottsdanker (1975):

> Let us consider an imaginary RT experiment on an astute S [subject]. After giving him a series of trials using a warning interval between 1 and 3 s, we omit the warning signal on one trial. His RT on that trial is 300 ms whereas his average had been 150 ms. We ask him why he was so slow. *"I was not prepared."* We then ask whether he does something to become prepared. *"Of course, I start paying attention and get ready to move my finger."* We question further whether it takes much time to become prepared. *"Oh, it is like doing anything else—something like tightening my grip on the steering wheel when I suddenly see a bump ahead."* Finally, we ask why he didn't simply stay prepared between trials. *"Are you serious? It takes real effort. You just can't be 'up' all the time."* (pp. 33–34)

The mechanism this view seems to endorse is that a critical moment is selected on the basis of high subjective expectancy and made the focus of intentional, even effortful, preparatory activity (e.g., Niemi & Näätänen, 1981). This mechanism is also clear from the notion of "repreparation," which, as explained earlier, fulfills a key role in the strategic view. Indeed, repreparation implies that as soon as participants notice that the imperative moment occurs later than the critical moment for which they initially prepared, they may use the remaining part of FP to reprepare for a later critical moment (Alegria, 1974; Loveless & Sanford, 1974).

In contrast, the conditioning view claims that within-trial preparation at the critical moments develops unintentionally, elicited by the occurrence of WS, which serves as the conditioned stimulus. The difference between this view and the strategic view is perhaps best appreciated by comparing the effortful preparatory activity reported by Gottsdanker's (1975) "astute subject" with the conditioned salivation response of Pavlov's (1927) dog. In fact, there is good empirical evidence that the conditioned response is elicited in spite of contrary intentions of the participant (Dawson & Reardon, 1969).

We emphasize, however, that the conditioning view does not imply that all processing within its bounds is devoid of intention. To make sure that this point is well taken, we now point out several senses in which the conditioning view does not exclude intentional contributions. First, the conditioning view does not imply that the adjustment of the state of conditioning is independent of the intentions of the participant. In fact, recent experimental findings at our laboratory suggest that this adjustment critically depends on the general intention of the participant to respond as quickly as possible to IS. Specifically, we found sequential effects to be much reduced after a trial on which the participant was instructed, before WS, not to respond to IS. Thus, when the participant has no intention to respond to IS, the state of conditioning corresponding to an early critical moment is much less suppressed when bypassed during FP than when the participant intends to respond to IS. This suggests that a general intention to respond is crucial to switch on the conditioning mechanism described in the previous section.

Second, our claim that response-related activation builds up unintentionally at critical moments should not be taken to imply that it also proceeds "automatically" in the sense that participants have no conscious awareness of the different critical moments in the design (cf. Dawson & Schell, 1985; Posner & Snyder, 1975). In fact, it is exceedingly difficult to demonstrate conditioning without participants being aware of the contingency between the conditioned and the unconditioned stimulus (see Dawson & Schell, 1985, for a comprehensive review and Öhman & Soares, 1998, for recent empirical work). Similarly, it may well be that both the acquisition and the production of the conditioned response described here require participants to be aware of the different critical moments in the design.

Third, and most important for present purposes, the conditioning view does not imply that intentional preparation cannot contribute to the state of nonspecific preparation. In fact, it seems quite likely that intentional preparation may enhance the preparatory state at a critical moment over and above the preparatory state deriving from conditioning alone. The conditioning view does imply, though, that a contribution of intentional preparation is not necessary for the observation of the typical (sequential) effects of FP.

As it turns out, then, the difference between the conditioning view and the strategic view concerning the role of intention is limited to the within-trial development of the state of preparation. Nevertheless, we think that this difference is theoretically important in that it underlies different preparatory mechanisms, as outlined in the previous sections.

Experimental Approach

To contrast the two major accounts of FP effects, we aimed at dissociating unintentional and intentional contributions to the state of nonspecific preparation. We modeled our experimental approach after the one used by Marcel and Forrin (1974) to examine the nature of the category-repetition effect. They had participants identify letters and digits that were randomly presented across the trials of each block. They observed the category-repetition effect, in which RTs were shorter for category repetitions (i.e., for a letter when preceded by a letter or for a digit when preceded by a digit) than for category alternations (i.e., for a letter preceded by a digit, or vice versa). In another experiment, Marcel and Forrin (1974, Experiment 3) presented a cue at the start of each trial informing the participant whether the impending category would be a letter or a digit. The cue was mostly valid but sometimes invalid, in which case it specified a digit before the actual presentation of a letter, or vice versa. Marcel and Forrin observed that whereas the category-repetition effect was almost absent on valid-cue trials, it was as strong on invalid-cue trials as on neutral-cue control trials.

These results suggest that the category-repetition effect, as observed on neutral-cue trials, does not result from an intentional preparatory strategy. The reason is as follows. The reduction of the category-repetition effect on valid-cue trials indicates that a participant is capable of intentionally enhancing his or her preparatory state for a specific category. However, if this intentional preparation also underlies the category-repetition effect, this effect should be eliminated not only on valid-cue trials but also on invalid-cue trials. The reason is that, if preparation is directed to the incorrect category, responding should be slow regardless of whether the category is the same as on the preceding trial or not. Therefore, as Marcel and Forrin (1974) concluded, observation of a pronounced category-repetition effect on invalid-cue trials indicates that this effect is unintentional in nature and reflects residual activation carried over from the preceding trial.

Following the approach of Marcel and Forrin (1974), we used two major variables: sequential order of FPs in mixed blocks and information about the duration of the impending FP, provided by a cue at the start of each trial. Across experiments, the cue was either *neutral,* when it provided no information about the duration of the impending FP, or *informative.* An informative cue was either *valid,* when it correctly specified the duration of the impending FP, or *invalid,* when it specified another FP than the one used on that trial. The task we asked our participants to perform was a choice-reaction task involving two compatible IS–response pairs and not a simple-reaction task involving a single IS–response pair, as is more customary in research on nonspecific preparation (e.g., Niemi & Näätänen, 1981). A choice-reaction task has perhaps the disadvantage of introducing uncertainty about the content of IS in addition to uncertainty about its timing, thereby detracting attention from the purpose of the study. On the other hand, in view of its accuracy requirements, a choice-reaction task discourages undesirable anticipatory response behavior more thoroughly than a simple-reaction task, even when catch trials (i.e., trials on which no IS occurs and the participant is required to withhold the response) are included. In any case, this issue is probably of minor importance given that many studies indicate that the effects of FP are at least qualitatively similar across the two types of tasks.[2]

Experiment 1

Experiment 1 was designed to replicate and extend previous findings. We presented FP in both pure and mixed blocks. In half of these blocks we presented the neutral cue, and in the other half we presented the valid cue. We also manipulated financial reward for fast and accurate responding to examine to what extent intentional preparation depends on the motivational state of the participant. Our predictions were as follows. First, in the neutral-cue condition, we expected to replicate the classical effects of FP on RT: the differential effect of FP in pure and mixed blocks and asymmetric sequential effects of FP in mixed blocks. Second, in the valid-cue condition, we expected a reduction of these effects consistent with the findings of a few studies in which preknowledge about the impending FP was varied (Kingstone, 1992, Experiment 4; Mo & Kersey, 1980; Zahn, 1970). Third, reasoning that this reduction depends on the motivation of the participant to prepare on the basis of the cue, we expected it to be stronger when fast and accurate responding was rewarded than when it was not rewarded.

We emphasize that the findings Experiment 1 sought to replicate are not yet diagnostic about the nature of nonspecific preparation. In particular, the expected outcome that participants are capable of preparing on the basis of a valid cue is not inconsistent with the conditioning view. It should be kept in mind that even though this view assumes that effects of FP derive first and foremost from an unintentional preparatory process, it does not exclude the possibility that an intentional preparatory process can contribute to these effects, as outlined in the previous section.

Method

Participants. Ten students between 19 and 23 years of age, all with normal or corrected-to-normal vision, volunteered as participants. They were paid 25 Dutch guilders (about $12) along with an additional bonus depending on their task performance.

Materials. The experiment was conducted on a computer equipped with a 486 DX2/66 processor and a 38.1-cm video graphics array color monitor. The software package ERTS was used for the layout and timing of the experimental trials (Beringer, 1992). The EXKEY interface connected the computer to an external response panel and allowed RT to be measured to the nearest millisecond. The response panel consisted of a 23-cm × 38-cm 10° tilted surface on which two microswitches were mounted, 3.5 cm from the upper side of the panel and 21.0 cm from each other. Each microswitch was covered by a round response button 2.5 cm in diameter. Participants sat at a distance of about 50 cm from the computer screen, with their left and right index fingers resting on the left and right response buttons, respectively. They wore padded headphones (Sennheisser, HD 250) through which the auditory WS was presented.

All visual stimuli were presented on the dark computer screen. The cue was a 1.5-cm × 1.0-cm vertical bar divided in three piled boxes 1.0 cm wide and 0.5 cm high. The contours of each box were yellow 0.1-cm line segments. In the neutral-cue condition, the boxes were uncolored and showed the dark computer screen. In the valid-cue condition, the boxes were colored yellow in accordance with the duration of one of three FPs. A coloring of only the bottom box indicated an impending FP of 0.5 s. An additional coloring of each subsequent box indicated impending FPs of 1.0 and 1.5 s, respectively. A cross, consisting of two yellow crossed bars 1.0 × 0.5 cm in size, served as a fixation stimulus. The IS was a downward pointing white arrow 2.0 cm long that consisted of a 1.5-cm × 1.0-cm bar attached to a 0.5-cm arrowhead with a maximum width of 2.0 cm.

Task. A trial started with the presentation of a valid or neutral cue on the middle of the screen. After 1,000 ms, the cue was replaced by the fixation stimulus. At the same time WS, a 1500 Hz pure tone, was presented for 50 ms over the headphones. The offset of WS marked the start of FP. When FP had expired, the fixation stimulus disappeared, and IS appeared with equal probability 4.5 cm to either the left or the right of the middle of the screen. Participants responded by making a spatially compatible button press. IS disappeared immediately after the response or after the expiration of a maximum interval of 1,500 ms, whichever occurred

[2] Eric Soetens suggested that, in a choice-reaction task, effects of FP may (partly) reflect differential sequential effects of IS–response pairs at different critical moments. Van den Heuvel (2000) examined this possibility in a data set very similar to the one reported in this study and observed a slightly shorter RT for intertrial alternations of IS–response pairs than for intertrial repetitions (consistent with findings of Soetens et al., 1985). Importantly, this effect was additive to the (sequential) effect of FP. For this reason, we collapsed our data across (intertrial transitions of) IS–response pairs in all of the experiments reported in this article.

earlier. After a blank interval of 100 ms, feedback appeared on the screen for 400 ms. During practice and in the no-payoff condition, feedback concerned accuracy only. In the case of a correct response the Dutch word *goed* (good) appeared in green, whereas in the case of an incorrect response the word *fout* (wrong) appeared in red. In the payoff condition, two additional feedback categories were used: *te snel* (too fast) and *te traag* (too slow) in red, when correct responses were below 100 ms and above an upper criterion (described subsequently), respectively. A blank interval of 400 ms separated subsequent trials.

Design and procedure. The independent variables were all varied within participants, and included FP (0.5, 1.0, or 1.5 s), block type (pure or mixed), cue type (neutral or valid), and reward (payoff or no payoff). In pure blocks only one FP was used across trials, whereas in mixed blocks the three possible FPs were randomly and equiprobably presented across trials. Cue type was varied between blocks, and reward was varied between two separate sessions. RT and percentage of errors (PE) were the dependent variables.

Participants were tested individually for about 3 hr. They were instructed to respond as quickly as possible while maintaining high accuracy. It was emphasized that the cue, whenever informative, always provided valid information. Furthermore, preceding each block of trials, participants read a text on the screen informing them of the impending cue type (neutral or valid), block type (pure or mixed), and, in the case of a pure block, FP duration. Participants initiated a block by pressing one of the response buttons.

Participants practiced six 12-trial pure blocks (two blocks with each FP) and two 14-trial mixed blocks, first with neutral cues and then with valid cues. They received visual feedback on mean RT and mean PE after each block. After the last practice block, there were two experimental sessions, one with payoff and one without payoff, the order of which was counterbalanced across participants. In the payoff session, the participant's median RT over the final 85 trials of the practice session served as an individual criterion. Participants started with 5.40 Dutch guilders. They lost 1 cent in the case of a correct response above the criterion and lost 2 cents in the case of an incorrect response or a correct response below 100 ms; otherwise, they earned 1 cent.

Either session consisted of 16 blocks of 62 trials each; 10 blocks were mixed and 6 blocks were pure, 2 with each FP. In either session, half of the pure and mixed blocks contained informative cues, and the other half contained uninformative cues. The order of blocks within a session was randomized for each participant. A brief interval was inserted between subsequent blocks during which participants received visual feedback on mean RT and mean PE, and, in the payoff session, on their cumulative financial reward. After each series of 8 experimental blocks, a 5-min break was inserted.

Data analysis. The data of the practice session were discarded, as were the first two trials of each block and trials on which the RT was either below 100 ms or above 1,000 ms. Mean RTs, computed over correct trials, and mean PEs were subjected to separate univariate analyses of variance (ANOVA) with repeated measures. The Huyn–Feldt correction was applied in all tests involving variables with more than two levels to correct for possible violations of the sphericity of the variance–covariance matrix (e.g., Stevens, 1992). An alpha level of .05 was used for all statistical tests.

Results

Pure versus mixed blocks. Figure 2 shows mean RT as a function of FP, block type, cue type, and reward. An ANOVA on these data yielded longer RTs in the session without payoff (253 ms) than in the session with payoff (230 ms), $F(1, 9) = 56.94$, $MSE = 569.67$, $p < .001$, and longer RTs in mixed blocks (244 ms) than in pure blocks (238 ms), $F(1, 9) = 21.56$, $MSE = 112.23$, $p < .01$. The main effects of cue type, $F(1, 9) = 3.59$, $MSE = 123.46$, $p > .05$, and FP, $F < 1$, were nonsignificant. There were

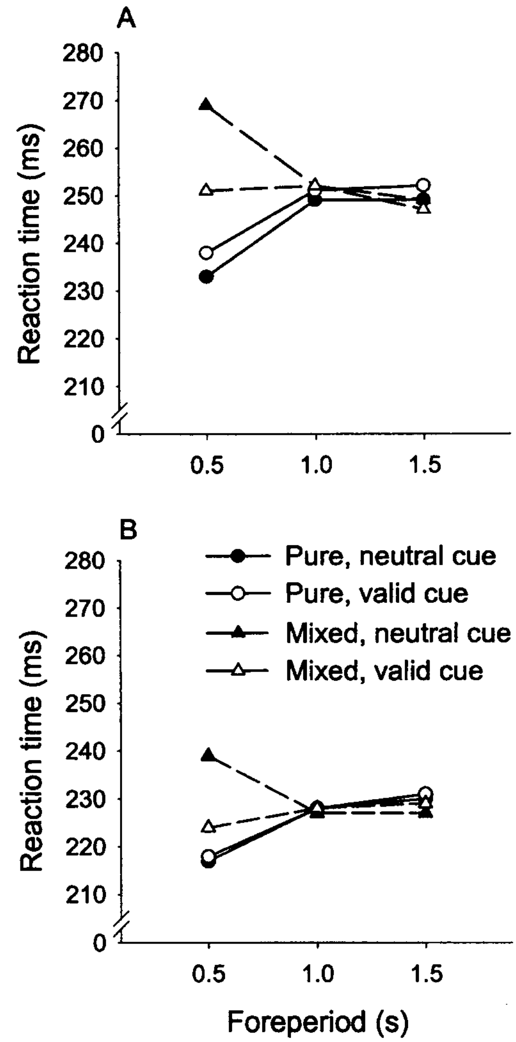

Figure 2. Mean reaction times in Experiment 1 as a function of foreperiod, block type, cue type, and financial reward. Panels A and B show the data for the no payoff and payoff conditions, respectively.

four significant two-way interactions. First, the interaction between block type and FP, $F(2, 18) = 23.93$, $MSE = 110.19$, $p < .001$, replicated the classical effect that RT increases with FP in pure blocks and decreases with RT in mixed blocks. Second, the interaction between block type and cue type, $F(1, 9) = 12.40$, $MSE = 58.71$, $p < .01$, reflects the fact that valid cues reduced RT in mixed blocks but not in pure blocks. Third, the interaction between reward and block type, $F(1, 9) = 6.78$, $MSE = 84.13$, $p < .05$, indicates that payoff was more effective in mixed blocks than in pure blocks. This differential effect of reward was limited to the shortest FP, as indicated by a significant three-way interaction among reward, block type, and FP, $F(2, 18) = 5.04$, $MSE = 39.31$, $p < .05$. Fourth, the interaction between FP and cue type, $F(2, 18) = 9.00$, $MSE = 73.46$, $p < .01$, indicates that the benefit of an informative cue decreased as FP increased. This interaction was clearly present in mixed blocks but not in pure blocks, as indicated by the significant interaction among block type, cue type, and FP, $F(2, 18) = 6.04$, $MSE = 64.24$, $p < .05$. There were no significant effects involving both reward and cue type, indicating that a valid cue flattened the FP–RT function in mixed blocks irrespective of reward. Despite this flattening, a small RT difference between pure and mixed blocks remained for the shortest FP in the valid-cue condition.

Overall PE averaged 1.37%. Table 1 shows mean PE as a function of FP, block type, cue type, and reward. An ANOVA on these data yielded no significant main effects. There was a significant interaction between block type and FP, $F(2, 18) = 7.15$, $MSE = 2.98$, $p < .01$, indicating that PE decreased with FP in pure blocks (2.34%, 1.04%, and 0.83% for FPs of 0.5, 1.0, and 1.5 s, respectively) but remained roughly the same across FP in mixed blocks (1.08%, 1.45%, and 1.45% for FPs of 0.5, 1.0, and 1.5 s, respectively). Thus, the reported increase of RT with FP in pure blocks seems, at least to some extent, due to shifts in the speed–accuracy trade-off. Finally, there was a significant interaction among reward, block type, cue type, and FP, $F(2, 18) = 3.75$, $MSE = 18.08$, $p < .05$; we do not attempt to interpret this interaction because the data did not show a consistent pattern.

Sequential effects. Figure 3 shows mean RT in mixed blocks as a function of FP_n (where n denotes the trial from which RT is sampled), FP_{n-1}, cue type, and reward. An ANOVA performed on these data revealed shorter RTs in the session with payoff (231 ms) than in the session without payoff (258 ms), $F(1, 9) = 43.44$, $MSE = 1431.11$, $p < .001$, and shorter RTs for valid cues (241 ms) than for neutral cues (248 ms), $F(1, 9) = 15.10$, $MSE = 256.58$, $p < .01$. Furthermore, RT decreased with FP_n, $F(2, 18) = 9.11$, $MSE = 314.30$, $p < .01$ (mean RTs were 250, 242, and 241 ms for FP_ns of 0.5, 1.0, and 1.5 s, respectively) and increased with FP_{n-1}, $F(2, 18) = 33.05$, $MSE = 160.28$, $p < .001$ (mean RTs were 238, 244, and 251 ms for FP_{n-1}s of 0.5, 1.0, and 1.5 s, respectively). A significant interaction between FP_n and FP_{n-1}, $F(4, 36) = 15.01$, $MSE = 80.05$, $p < .001$, indicates that the effect of FP_{n-1} decreased as FP_n increased. As Figure 3 shows, the effect of FP_{n-1} was very strong when FP_n was 0.5 s and virtually absent when FP_n was either 1.0 or 1.5 s. Cue type interacted both with FP_n, $F(2, 18) = 53.06$, $MSE = 58.77$, $p < .001$, and with FP_{n-1}, $F(2, 18) = 13.95$, $MSE = 44.36$, $p < .001$. In turn, these variables were involved in a significant three-way interaction, $F(4, 36) = 16.00$, $MSE = 46.49$, $p < .001$. As Figure 3 shows, the presentation of a valid cue strongly reduced the sequential effect for the shortest FP_n but not for the longer FP_ns. In spite of this reduction, the effect of FP_{n-1} for the shortest FP_n in the valid-cue condition (averaged across the levels of reward) was still significant, $F(2, 18) = 6.34$, $MSE = 82.17$, $p < .01$. Finally, the interaction between reward and FP_{n-1} approached significance, $F(2, 18) = 3.47$, $MSE = 74.35$, $p = .053$, indicating that the effect of the preceding FP tended to be somewhat less pronounced in the payoff condition than in the condition without payoff.

Table 2 shows mean PE in mixed blocks as a function of FP_n, FP_{n-1}, cue type, and reward. An ANOVA on these data was not possible, because there was no variance in one condition in which all participants had faultless performance. Therefore, we merely note that, across conditions, mean PE was 1.35%, and the range was 0% to 3.35% among conditions.

Table 1
Error Percentages in Experiment 1 as a Function of Foreperiod (in Seconds), Block Type, Cue Type, and Reward

Foreperiod	Pure block: Neutral cue	Pure block: Valid cue	Mixed block: Neutral cue	Mixed block: Valid cue
		No payoff		
0.5	2.17	2.51	1.50	1.31
1.0	0.67	0.83	1.70	1.20
1.5	0.83	1.00	0.91	1.81
		Payoff		
0.5	2.18	2.51	0.20	1.30
1.0	2.00	0.67	1.30	1.61
1.5	0.83	0.68	1.80	1.30

Discussion

Experiment 1 replicated classical effects of FP in pure and mixed blocks (e.g., Niemi & Näätänen, 1981; Woodrow, 1914). In the neutral-cue condition, RT increased with FP in pure blocks and decreased with FP in mixed blocks. This difference derived, at least to a large extent, from sequential effects in mixed blocks, where RT was longer for a given FP when it was preceded by a longer FP than when it was preceded by an equally long or shorter FP.[3] In regard to the shortest FP, Experiment 1 also replicated earlier findings concerning the role of preknowledge (Kingstone, 1992; Mo & Kersey, 1980; Zahn, 1970). For this FP, participants proved capable of reducing RT in mixed blocks relative to that in pure blocks when the trial started with a valid cue. In turn, this reduction was, to a large extent, due to a near elimination of sequential effects in mixed blocks.

Payoff proved to be an excellent motivator in that it considerably enhanced performance in general. On the assumption that the extent of preparation on the basis of a valid cue depends on the motivational state of the participant (e.g., De Jong, 2000), we expected a greater effect of cue type in the payoff condition than in the condition without payoff. However, the relevant interaction between cue type and reward was far from significant. Inspection of the data corresponding to the shortest FP (i.e., Figure 3A) suggests a clear reason for this result. Participants made excellent use of the valid cue, even without monetary incentive, as is apparent from the near elimination of sequential effects in the no-payoff, valid-cue condition. Therefore, in the payoff condition, there was not much room for further enhancement of the state of preparation at the imperative moment.

In conclusion, the results of Experiment 1 show that participants are capable of intentional preparation for a specific imperative moment, even in a state of less than optimal motivation. This result is consistent with the strategic view of preparation, although it is not inconsistent with the conditioning view, as explained in the Experimental Approach section.

[3] Whether sequential effects can account for all of the systematic variance in RT caused by block type is difficult to assess, because it requires higher order sequential effects in mixed blocks to be taken into account (e.g., Drazin, 1961; Los et al., in press), along with differences between pure and mixed blocks regarding the speed–accuracy trade-off (e.g., Bertelson, 1967; Los et al., in press).

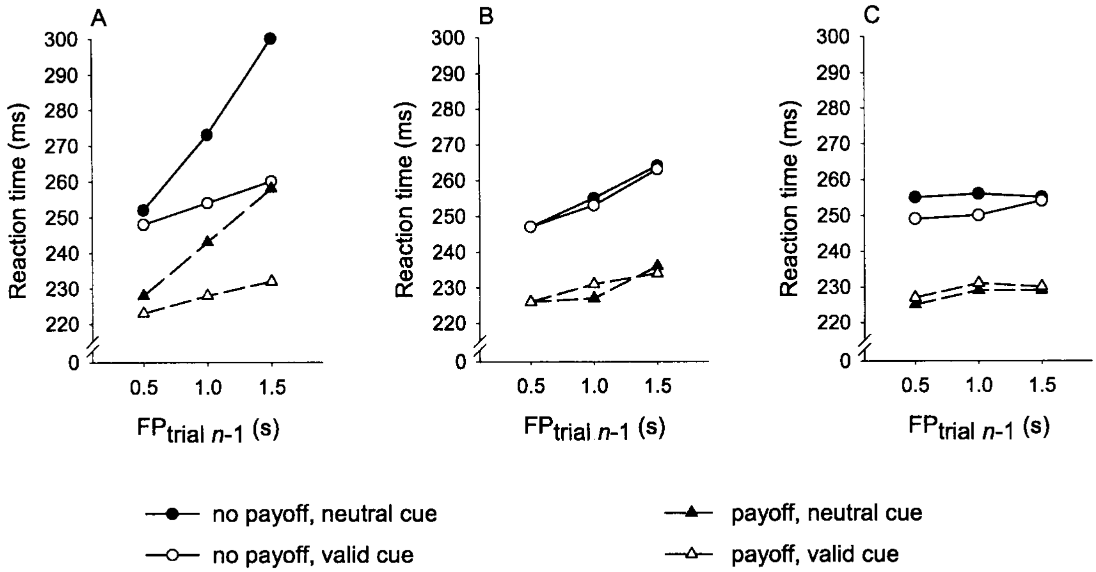

Figure 3. Mean reaction times in mixed blocks of Experiment 1 as a function of $FP_{trial\ n}$ (the FP occurring on the trial from which reaction time was sampled), $FP_{trial\ n-1}$, cue type, and financial reward. FP = foreperiod. Panels A, B, and C show the data for $FP_{trial\ n}$ s of 0.5 s, 1.0 s, and 1.5 s, respectively.

Finally, two findings of Experiment 1 pose problems for both the strategic view and the conditioning view. First, RT for the shortest FP was longer when the longest FP occurred on the preceding trial than when the middle FP occurred on the preceding trial. This effect is not in accordance with the strategic view, because in both conditions participants are supposed to prepare for a critical moment beyond the earliest critical moment, such that repreparation is to no avail when the earliest critical moment becomes imperative. This effect is also inconsistent with the conditioning view, because the model we described has no proviso that extinction of the state of conditioning corresponding to a critical moment depends on the extent to which that critical moment is bypassed during FP on the preceding trial. Second, the effects of block type and sequential effects in mixed blocks were pronounced for the shortest FP but were hardly present for longer FPs. Both the strategic view and the conditioning view predicted this result for the longest FP but not for the middle FP. According to the strategic view, after a trial with the longest FP, participants are caught in a low preparatory state when the middle critical moment becomes imperative, and repreparation is to no avail. According to the conditioning view, the state of conditioning corresponding to the middle critical moment is extinguished when the longest FP was used on the preceding trial, also leading to a low preparatory state. Both of these findings suggest that contributions to the state of nonspecific preparation corresponding to a critical moment, whether deriving from intentional preparation or conditioning, are not as discrete as assumed so far but smeared over time. We elaborate on this view in the General Discussion section.

For the moment, the second finding makes it clear that the effect of cue type can be meaningfully studied only for the shortest FP. Indeed, if the state of nonspecific preparation is not affected by the preceding FP, it makes no sense to explore a possible modifying influence of cue type. Therefore, in the following experiments, we limit the discussion of cue type to the shortest FP (see also Kingstone, 1992, Experiment 4; Mo & Kersey, 1980; Zahn, 1970).

Experiment 2

Experiment 1 showed that participants are capable of intentional preparation for a specific imperative moment when they have preknowledge about the impending FP in mixed blocks. However, this result is not conclusive about the nature of the nonspecific preparation underlying the effect of FP. On the one hand, it is possible that the same intentional preparatory process underlies the effect of both cue type and FP, as follows from the strategic view. On the other hand, it is possible that the contribution of an intentional preparatory process to the state of nonspecific preparation obscures the contribution of an unintentional preparatory process, as follows from the conditioning view.

Experiment 2 was aimed at unraveling intentional and unintentional contributions to the state of nonspecific preparation. We presented FPs in mixed blocks only, with valid cues occurring on 80% of the trials and invalid cues occurring on the remaining trials. Because the cue was generally valid, we expected participants to direct their preparation to the specified critical moment. Therefore, we predicted only minimal sequential effects in the valid-cue condition, consistent with the findings of Experiment 1. The strategic view and the conditioning view share this prediction but make different predictions about performance in the invalid-cue condition. The strategic view predicts that when IS occurs earlier than specified by the cue (such that repreparation is not possible), there should be only a general cost on RT and no sequential effects exceeding those observed in the valid-cue condition. This is because the distraction of intentional preparation to a critical moment beyond the imperative moment should cause the basis of sequential effects to disappear. By contrast, the conditioning view predicts that the cost inflicted by the invalid cue depends on the FP

Table 2
Error Percentages in Mixed Blocks of Experiments 1, 2, and 3 as a Function of FP_n, FP_{n-1} (Both in Seconds), and Cue Type

Cue type	0.5–0.5	0.5–1.0	0.5–1.5	1.0–0.5	1.0–1.0	1.0–1.5	1.5–0.5	1.5–1.0	1.5–1.5
	FP_n–FP_{n-1}								
Experiment 1									
Neutral									
No payoff	0.55	1.54	2.54	1.34	1.14	2.79	0.90	0.27	1.13
Payoff	0.00	0.28	0.40	1.59	1.01	1.37	3.35	0.62	1.49
Valid									
No payoff	0.82	0.98	2.39	0.89	1.23	1.25	2.07	2.63	0.97
Payoff	1.18	0.92	1.58	2.99	0.57	1.44	0.61	1.80	1.91
Experiment 2									
Valid	1.86	2.60	1.93	2.68	2.54	1.74	2.08	2.84	2.52
Invalid	1.85	1.47	2.14	2.20	2.08	2.31	3.03	2.80	3.19
Experiment 3									
Neutral	1.70	3.42	4.99	2.00	2.02	2.53	2.55	3.31	1.41
Valid	2.86	2.79	2.32	2.25	2.32	1.90	2.31	2.47	2.04
Invalid	2.39	2.34	2.36	2.36	2.56	1.65	3.33	2.96	2.59

Note. Reward was varied in Experiment 1 and fixed (payoff only) in Experiments 2 and 3. FP = foreperiod; n = trial from which reaction time is sampled.

occurring on the preceding trial because the distraction of intentional preparation from the imperative moment should reveal the state of conditioning corresponding to that moment in its uncontaminated form.

Method

Twenty-one students between 18 and 23 years of age, with normal or corrected-to-normal vision, participated. None of them had participated in Experiment 1. They were paid 25 Dutch guilders along with an additional bonus depending on their performance. The apparatus, stimuli, and FPs, as well as the order of events on a trial, were identical to those of Experiment 1. FPs were presented in mixed blocks only, with an informative cue occurring at the start of each trial. The cue provided valid information about the duration of the impending FP on a random 80% of the trials. On the remaining 20% of the trials, the cue provided invalid information and specified with an equal probability the presentation of one of the two other possible FPs. Participants were informed that the cue was usually but not always valid. It was stressed, however, that regardless of cue validity, they should try to respond as fast and as accurately as possible. Participants received a single practice block of 68 trials during which the cue always conveyed valid information, and feedback was given on accuracy only. Then they completed, in a single experimental session, 15 mixed blocks of 122 trials each, with 5-min breaks following each series of 5 blocks. Median RT over the final 60 trials of the practice block served as an individual criterion in the payoff system used in the experimental session. Participants started with 3.00 Dutch guilders. They lost 0.25 cents in the case of a correct response that was slower than the criterion and lost 0.50 cents in the case of an incorrect response or a response below 100 ms; otherwise, they earned 0.25 cents. In all other respects, the method was identical to that of Experiment 1.

Results

Figure 4 shows RT as a function of cue validity (valid or invalid), FP_n (0.5, 1.0, or 1.5 s), and FP_{n-1} (0.5, 1.0, or 1.5 s). An ANOVA performed on these data revealed significant effects of FP_n, $F(2, 40) = 40.32$, $MSE = 221.40$, $p < .001$, and FP_{n-1}, $F(2, 40) = 49.00$, $MSE = 106.15$, $p < .001$, as well as an interaction between FP_n and FP_{n-1}, $F(4, 80) = 17.54$, $MSE = 83.44$, $p < .001$. These effects replicated those of Experiment 1. Furthermore, there was a significant main effect of cue validity, $F(1, 20) = 23.56$, $MSE = 121.44$, $p < .001$, indicating that responding was slightly faster when the cue provided valid information (243 ms) than when it provided invalid information (248 ms). There was a significant interaction between cue validity and FP_n, $F(2, 40) = 44.16$, $MSE = 47.94$, $p < .001$, indicating that the effect of cue validity was present for the shortest FP_n but not for the other two FP_ns. The interaction between cue validity and FP_{n-1} was not significant, $F(2, 40) = 2.78$, $MSE = 29.17$, $p > .05$. Finally, there was a significant three-way interaction among cue validity, FP_n, and FP_{n-1}, $F(4, 80) = 4.42$, $MSE = 29.04$, $p < .01$. Tests of the specific interaction effects for the separate FP_ns revealed that there was a significant two-way interaction between cue validity and FP_{n-1} for the shortest FP_n, $F(2, 40) = 9.64$, $MSE = 32.94$, $p < .001$, but not for the other two FP_ns ($F < 1$ in both cases). This effect is also clear from Figure 4. As in Experiment 1, the specific main effect of FP_{n-1} for the shortest FP_n in the valid-cue condition was still highly significant, $F(2, 40) = 17.85$, $MSE = 116.67$, $p < .001$.

Overall PE averaged 2.37%. Table 2 shows mean PE as a function of cue validity, FP_n, and FP_{n-1}. The ANOVA on these data yielded a significant main effect of FP_n, $F(2, 40) = 5.06$, $MSE = 5.12$, $p < .05$, indicating that PE increased with FP_n (1.98%, 2.26%, and 2.86% for FP_ns of 0.5 s, 1.0 s, and 1.5 s, respectively). This suggests that some part of the corresponding effect on RT may have been attributable to shifts in speed–accuracy trade-off. No other effects on PE were significant.

In a final analysis we examined, for the shortest FP_n (Figure 4A), whether there were effects on RT of the specific invalid cue (specifying an FP of either 1.0 or 1.5 s). The ANOVA with cue

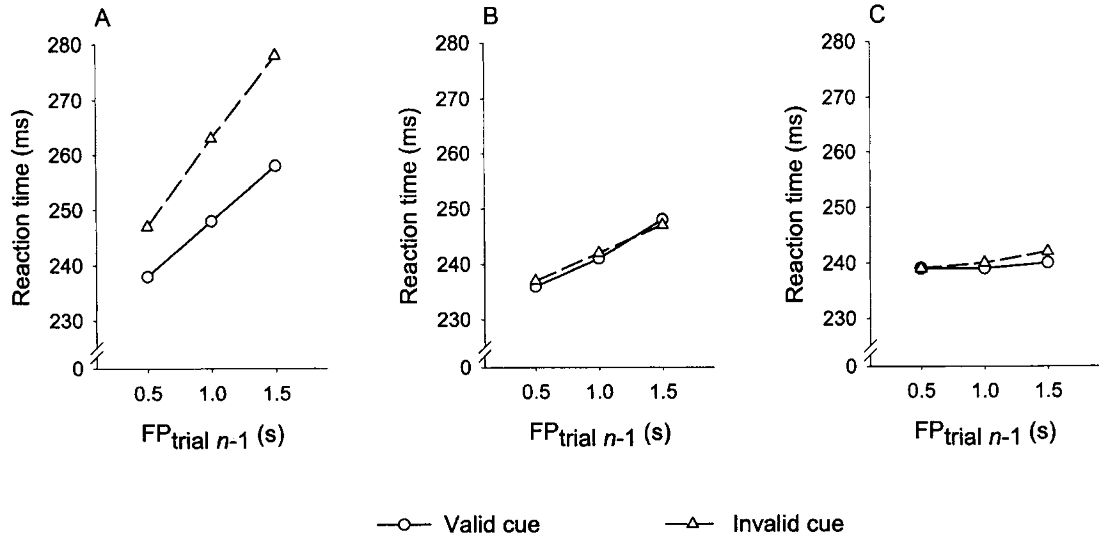

Figure 4. Mean reaction times in Experiment 2 as a function of $FP_{trial\ n}$ (the FP occurring on the trial from which reaction time was sampled), $FP_{trial\ n-1}$, and cue validity. FP = foreperiod. Panels A, B, and C show the data for $FP_{trial\ n}$ s of 0.5 s, 1.0 s, and 1.5 s, respectively.

type (1.0 s or 1.5 s) and FP_{n-1} (0.5 s, 1.0 s, or 1.5 s) as variables revealed a main effect of cue type, $F(1, 20) = 4.46$, $MSE = 198.52$, $p < .05$, indicating that responding was slightly faster when the cue specified an FP_n of 1.0 s (260 ms) than when it specified an FP_n of 1.5 s (265 ms). Cue type did not interact with FP_{n-1} ($F < 1$).

Discussion

Experiment 2 replicated the classical interaction between FP_n and FP_{n-1}, a result that was also observed in Experiment 1. This interaction depended in turn on cue validity: Cue validity modified the effect of FP_{n-1} for the shortest FP_n but not for the longer FP_ns. For reasons indicated in the Discussion section of Experiment 1, we limit our discussion to the findings for the shortest FP_n (see Figure 4A).

Regarding the shortest FP_n, two findings are important. First, RT was shorter in the valid-cue condition than in the invalid-cue condition. This shows that participants aimed their focus of intentional preparation to the critical moment specified by the cue. Second, the effect of FP_{n-1} was stronger in the invalid-cue condition than in the valid-cue condition. We anticipated that this finding would favor the conditioning view over the strategic view. According to the strategic view, the strategy of preparing on the basis of the cue replaces the strategy of preparing on the basis of FP_{n-1} (i.e., the strategy suggested by the strategic view in the absence of an informative cue). Therefore, to the extent that sequential effects occur at all, they should not be any larger in the invalid-cue condition than in the valid-cue condition, contrary to our finding. By contrast, according to the conditioning view, focusing intentional preparation on an incorrect critical moment inflicts a cost on RT only to the extent that the conditioned activation corresponding to the imperative moment is low. Specifically, this cost should be low in the case in which the imperative moment was also imperative on the preceding trial and high in the case in which it was bypassed on the preceding trial, consistent with our finding.

It is important to note, though, that the predictions deriving from the strategic view and the conditioning view are accurate only insofar as participants consistently prepare on the basis of the cue on every trial. To see this, suppose that participants prepare on the basis of the cue on only, say half of the trials, but ignore the cue and prepare for the critical moment that was imperative on the preceding trial on the other half of the trials. In that case, the strategic view would also predict an interaction between cue validity and FP_{n-1}. Unfortunately, the data of Experiment 2 suggest that participants may have followed such a mixture strategy. In particular, there was a considerable effect of FP_{n-1} even in the valid-cue condition. If participants always prepared on the basis of the information provided by the cue, this effect should have been absent or very small, as was the case in Experiment 1.

Experiment 3

The purpose of Experiment 3 was to provide a stronger test of the nature of nonspecific preparation along the lines of Experiment 2. For this purpose, we repeated the main conditions of Experiment 2 with the following changes. First, the percentage of trials on which the cue was invalid was lowered from 20% to 10%. By making the cue more reliable, participants were encouraged to base their preparatory strategy on the information provided by the cue. Second, we included blocks in which the cue was neutral to examine how sequential effects in this condition compared with those in the valid-cue and invalid-cue conditions.

Consistent with Experiment 1, we expected that sequential effects for the shortest FP_n would be smaller in the valid-cue condition than in the neutral-cue condition. Relative to the sequential effects in these conditions, the sequential effects in the invalid-cue condition should provide insight into the nature of nonspecific preparation. According to the strategic view, sequential effects in the invalid-cue condition should be equal to those in the valid-cue condition. As explained earlier, this is because an incorrect preparatory focus also causes the basis for sequential effects to disappear. By contrast, according to the conditioning view, sequential effects in the invalid-cue condition should be equal to those in the neutral-cue condition. This is because intentional preparation for a specific critical moment is presumed not to occur in the neutral-cue condition, whereas it is distracted from the imperative moment in the invalid-cue condition. In either case, sequential effects should solely reflect the state of conditioning corresponding to the imperative moment.

Method

Twelve students between 19 and 23 years of age, all with normal or corrected-to-normal vision, participated. None of them had participated in one of the previous experiments. They were paid 100 Dutch guilders along with an additional bonus depending on their performance. The apparatus, stimuli, and FPs, as well as the order of events on a trial, were identical to those of Experiment 2. FPs were presented in mixed blocks, whereas informative and neutral cues were varied between blocks. The informative cue was valid on a random 90% of the trials and invalid on the remaining 10% of the trials. Participants came to the laboratory on 4 different days within a period of 2 weeks and completed 56 experimental blocks of 122 trials each, 49 blocks with informative cues and 7 blocks with neutral cues. On the 1st day, they initially received task instruction and two 54-trial practice blocks, followed by 8 experimental blocks. On the other 3 days, participants started with one 54-trial practice block and then completed 16 experimental blocks. In all sessions, each series of 8 blocks contained 1 block with neutral cues at a random position relative to 7 blocks with informative cues. During practice on the 1st day, feedback was given on both accuracy and speed, with a speed criterion of 500 ms. Median RT for the second practice block served as an individual criterion in the payoff system throughout the experimental sessions. Participants started with 2.50 Dutch guilders. They earned 0.50 cents for each correct response between 100 ms and the criterion. Otherwise, they neither earned nor lost money. Participants received visual feedback on performance and their current money reward after every second block, and there was a 5-min break after every eighth block. In all other respects, the method was identical to that of Experiment 2.

Results

Figure 5 shows RT as a function of cue type (neutral, valid, or invalid), FP_n (0.5, 1.0, or 1.5 s), and FP_{n-1} (0.5, 1.0, or 1.5 s). An ANOVA performed on these data revealed significant effects of FP_n, $F(2, 22) = 28.49$, $MSE = 166.99$, $p < .001$, and FP_{n-1}, $F(2, 22) = 73.33$, $MSE = 30.06$, $p < .001$, as well as an interaction between FP_n and FP_{n-1}, $F(4, 44) = 24.85$, $MSE = 15.57$, $p < .001$, replicating the results of Experiments 1 and 2. Furthermore, there was a main effect of cue type, $F(2, 22) = 6.89$, $MSE = 132.16$, $p < .01$, indicating that overall responding was slightly faster when the cue was neutral (225 ms) or valid (223 ms) than when the cue was invalid (229 ms). Cue type interacted significantly with FP_n, $F(4, 44) = 15.19$, $MSE = 83.64$, $p < .01$, indicating that its effect was much stronger for the shortest FP_n than for the other two FP_ns, consistent with the results of Experiment 2. Cue type also interacted significantly with FP_{n-1}, $F(4, 44) = 6.36$, $MSE = 9.37$, $p < .001$, indicating that its effect increased with FP_{n-1}. As in Experiment 2, there was a significant three-way interaction among cue type, FP_n, and FP_{n-1}, $F(8, 88) = 4.62$, $MSE = 20.31$, $p < .01$. This interaction indicates that cue type strongly modifies the effect of FP_{n-1} for the shortest FP_n but not for the longer FP_ns.

To examine the data for the shortest FP_n (Figure 5A) in greater detail, we performed a separate ANOVA on these data with FP_{n-1} and cue type as variables. This analysis revealed significant effects of FP_{n-1}, $F(2, 22) = 63.77$, $MSE = 33.27$, $p < .001$, and cue type, $F(2, 22) = 12.44$, $MSE = 269.70$, $p < .01$, as well as a significant interaction between these variables, $F(4, 44) = 6.78$, $MSE = 27.04$, $p < .001$. Tests for simple main effects of cue type showed that relative to RT for the neutral cue (234 ms), RT for the

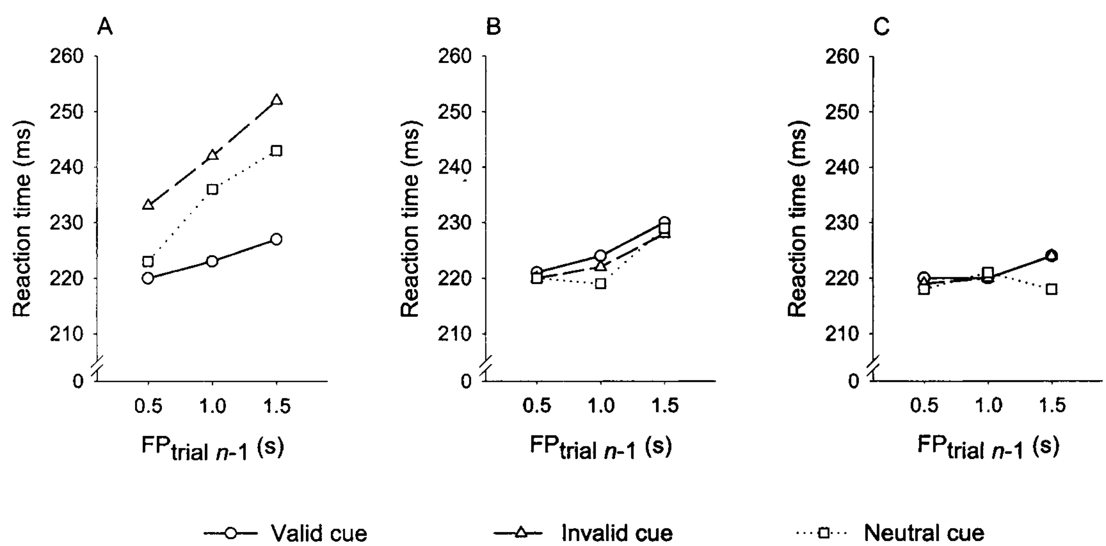

Figure 5. Mean reaction times in Experiment 3 as a function of $FP_{trial\ n}$ (the FP occurring on the trial from which reaction time was sampled), $FP_{trial\ n-1}$, and cue type. FP = foreperiod. Panels A, B, and C show the data for $FP_{trial\ n}$ s of 0.5 s, 1.0 s, and 1.5 s, respectively.

valid cue (223 ms) was significantly shorter, $F(1, 11) = 16.12$, $MSE = 772.75$, $p < .01$, whereas RT for the invalid cue (242 ms) was significantly longer, $F(1, 11) = 6.89$, $MSE = 1,137.87$, $p < .05$. The simple main effect of FP_{n-1} in the valid-cue condition was also significant, $F(2, 22) = 13.73$, $MSE = 10.74$, $p < .001$, consistent with the results of Experiments 1 and 2. Furthermore, we tested the interaction between FP_{n-1} and cue type for each pair of cues. The interaction was significant whenever the valid cue was part of the pair: valid versus neutral, $F(2, 22) = 16.92$, $MSE = 17.04$, $p < .001$, and valid versus invalid, $F(2, 22) = 7.81$, $MSE = 28.26$, $p < .05$. By contrast, the interaction was not significant for the comparison between the neutral cue and the invalid cue, $F(2, 22) = 1.14$, $MSE = 35.82$, $p > .30$.

Finally, we examined whether RT for the shortest FP_n was affected by the specific invalid cue (specifying an FP of either 1.0 or 1.5 s) relative to the neutral-cue condition. The ANOVA with cue type (neutral, 1.0 s, or 1.5 s) and FP_{n-1} (0.5 s, 1.0 s, or 1.5 s) as variables yielded main effects of FP_{n-1}, $F(2, 22) = 39.02$, $MSE = 86.64$, $p < .001$, and cue type, $F(2, 22) = 8.31$, $MSE = 193.40$, $p < .01$, but no interaction between these variables, $F(4, 44) = 1.35$, $MSE = 71.61$, $p > .25$. Tests for simple effects revealed that RT was significantly longer for the cue specifying an FP of 1.5 s (247 ms) than for either the neutral cue (234 ms), $F(1, 11) = 9.70$, $MSE = 1,895.03$, $p < .01$, or the cue specifying an FP of 1.0 s (238 ms), $F(1, 11) = 10.25$, $MSE = 855.45$, $p < .01$. The RTs for the neutral cue and the cue specifying an FP of 1.0 s did not differ significantly, $F(1, 11) = 2.41$, $MSE = 730.68$, $p > .10$.

Table 2 shows PE as a function of cue type (neutral, valid, or invalid), FP_n (0.5, 1.0, or 1.5 s), and FP_{n-1} (0.5, 1.0, or 1.5 s). An ANOVA performed on these data revealed no significant main effects, but there were significant interactions between FP_n and FP_{n-1}, $F(4, 44) = 3.18$, $MSE = 2.15$, $p < .05$; between FP_{n-1} and cue type, $F(4, 44) = 2.74$, $MSE = 2.01$, $p < .05$; and among FP_n, FP_{n-1}, and cue type, $F(8, 88) = 4.00$, $MSE = 1.49$, $p < .01$. All of these interactions basically reflect a strong increase in PE with FP_{n-1} for the shortest FP_n in the neutral-cue condition, whereas PE varied much less across FP_{n-1} in other conditions.

Discussion

Experiment 3 replicated the central findings of the two preceding experiments. Specifically, the effect of FP_{n-1} for the shortest FP_n was considerably smaller in the valid-cue condition than in either the neutral-cue condition or the invalid-cue condition, consistent with the findings of Experiments 1 and 2, respectively. Furthermore, beyond the shortest FP_n, there were only minimal effects of FP_{n-1} or cue type, consistent with the findings of both Experiment 1 and Experiment 2. As before, we focus in what follows on the findings for the shortest FP_n.

In addition to replicating earlier findings, Experiment 3 yielded two strong indications that participants consistently prepared for action at the critical moment specified by the informative cue. First, the effect of FP_{n-1} was almost reduced to zero in the valid-cue condition, which reveals intentional preparation for action at the correctly specified imperative moment. The residual effect was considerably less pronounced than that observed in Experiment 2, which suggests that the higher proportion of valid cues in Experiment 3 encouraged participants to base their preparatory strategy on the informative cue. In fact, the residual effect was approximately equal to that observed in Experiment 1, in which the cue was always valid. This suggests that the informative cue was treated as if it were always valid. Second, relative to the neutral-cue condition, there was a general RT cost in the invalid-cue condition. This finding is important because it makes it unlikely that participants allowed their preparatory strategy to depend on the FP specified by the informative cue. For instance, one such strategy would be to prepare on the basis of the cue when it specifies the shortest FP, reasoning that there is time for repreparation if the cue proves invalid, but to ignore the cue when it specifies a longer FP so as to avoid slow responding when the earliest critical moment becomes imperative. However, if this were the strategy participants followed, there is no basis for the RT cost in the invalid-cue condition relative to the neutral-cue condition. The finding that this cost occurred for the invalid cue specifying an FP of 1.5 s but not for an invalid cue specifying an FP of 1.0 s is taken up in the General Discussion section.

Because participants consistently prepared on the basis of the informative cue, the results of Experiment 3 favor the conditioning view of nonspecific preparation over the strategic view. According to the strategic view, the effect of FP_{n-1} observed in the neutral-cue condition indicates that uncertainty about the impending FP induces a strategy of preparing for action at the critical moment that was imperative on the preceding trial. Therefore, a strong reduction of this effect should be observed when participants prepare on the basis of the informative cue, regardless of its validity. However, as Figure 5A shows, the effect of FP_{n-1} in the invalid-cue condition clearly exceeded that in the valid-cue condition. By contrast, according to the conditioning view, the effect of FP_{n-1} in the neutral-cue condition reflects the participant's state of nonspecific preparation as it is adjusted by a process of trace conditioning. This conditioning effect may be obscured in the valid-cue condition, when intentional preparation contributes to the state of nonspecific preparation at the imperative moment, but should recur in the invalid-cue condition, when intentional preparation is distracted to a critical moment beyond the imperative moment. This is in agreement with the observation that the effect of FP_{n-1} was about as large in the invalid-cue condition as in the neutral-cue condition.

General Discussion

This study tested two views of the nature of nonspecific preparation: the strategic view and the conditioning view. These views differ with respect to the role of intention in the within-trial development of the state of nonspecific preparation corresponding to a critical moment. According to the strategic view, this development requires an intentional preparatory process that is guided by the expectancy of the participant as to which of several critical moments is going to be imperative on a given trial (e.g., Alegria, 1975; Gottsdanker, 1975; Niemi & Näätänen, 1981). By contrast, according to the conditioning view, the development of the preparatory state is identified with a conditioned response that is unintentionally elicited by WS (Los, 1996; Los et al., 2001).

To dissociate intentional and unintentional contributions to the state of nonspecific preparation, we first assessed to what extent participants are capable of intentional preparation for action at a moment specified by a cue. Experiment 1 showed that participants are indeed very capable of doing this. As compared with performance in a neutral-cue control condition, a valid cue caused a near elimination of sequential effects in mixed blocks as well as a strong reduction of the RT difference between pure and mixed blocks. Although these findings show that intentional preparation can contribute to the state of nonspecific preparation, they do not imply that the effects of FP observed in the neutral-cue condition originate from a similar preparatory process. It is perfectly possible that the provision of a valid cue encourages participants to make an intentional contribution to a state of nonspecific preparation that is normally regulated by conditioning processes alone. To examine this hypothesis, we added invalid cues on a small proportion of the trials in Experiments 2 and 3. In both experiments, sequential effects proved stronger in the invalid-cue condition than in the valid-cue condition. In particular, the results of Experiment 3 showed that whereas sequential effects were almost eliminated in the valid-cue condition, they were as strong in the invalid-cue condition as in the neutral-cue control condition. Thus, it appears that sequential effects occur when the focus of intentional preparation is distracted from the imperative moment. This dissociation of an unintentional contribution to the state of nonspecific preparation from an intentional one is predicted by the conditioning view but not by the strategic view.

In conclusion, the present findings suggest that intentional control is not the hallmark of nonspecific preparation during FP and that the maintenance of nonspecific preparation is not well characterized as an "aversive state" (Gottsdanker, 1975; Näätänen, 1972). Instead, the data indicate that nonspecific preparation is regulated unintentionally by a process of trace conditioning. Even though we have shown that an intentional preparatory process can contribute to the state of nonspecific preparation, participants seem to make this contribution only when explicitly encouraged to do so. In other cases, the intentions of the participant do not seem to pertain to the timing of IS.

Evidence for Path Independence

A basic assumption underlying our experimental approach was what is sometimes called path independence (e.g., Roberts, 1998, p. 72). According to this assumption, behavior is a function of the magnitude of some internal state, irrespective of how that state was reached. Applied to our case, RT is a function of the state of nonspecific preparation and not of the relative contribution of conditioning processes and strategic processes to this state. The findings of the present study were consistent with this assumption. To observe fast responding it proved sufficient to have a strong contribution of either the conditioning process or the strategic process, whereas to observe slow responding it was necessary that both of these contributions were low.

Converging evidence for path independence was recently obtained by Coull, Frith, Büchel, and Nobre (2000) using functional magnetic resonance imaging (fMRI). The experimental design in their study was basically the same as in the present study, with two FPs, of 600 and 1,400 ms, presented in mixed blocks of trials and a cue specifying the impending FP with a valid–invalid ratio of 4:1. The behavioral findings were consistent with the present findings. In the valid-cue condition, RT was about equal for short and long FPs; in the invalid-cue condition, however, RT was considerably longer for the short FP than for the long FP. Sequential effects were not reported. The fMRI data revealed different anatomical areas underlying motor preparation, as identified by comparing brain activation in the short and long FP conditions (for valid cues only), and temporal orientation, as identified by comparing brain activation in the valid-cue and invalid-cue conditions (averaged across FP). Motor preparation involved the left anterior putamen, the bilateral thalamus, and the supplementary motor area, whereas temporal orientation involved the inferior premotor–prefrontal and orbitofrontal cortex bilaterally, the left insula, and the left inferior parietal cortex. The differential implication of prefrontal areas in these two activities led Coull et al. to conclude that motor preparation is essentially an unintentional, bottom-up process, whereas temporal orientation is an intentional, top-down process. They stated that "although motor preparation or timing may be intimately linked to temporal attentional orienting from a conceptual viewpoint, anatomically they can be dissociated" (Coull et al., 2000, p. 816). Perhaps, then, our interpretation of motor preparation (i.e., as reflected by sequential effects of FP) in terms of conditioning processes provides the missing link in a conceptual framework corresponding to this anatomical distinction.

Finally, regarding the intentional contribution to the state of nonspecific preparation, the picture we have drawn is probably not complete. In both Experiments 2 and 3, we observed that, for the shortest FP, RT was slower when the invalid cue specified an FP of 1.5 s than when it specified an FP of 1.0 s. Importantly, in both experiments this effect was additive to the effect of FP_{n-1}. A possible interpretation of this finding, suggested by application of the additive-factors method (Sternberg, 1969), is that the specific invalid cue affects a nonmotor stage of information processing. Again, brain-imaging studies lend some support to this interpretation. In their fMRI study, Coull et al. (2000) observed exclusively in visual-cortex areas a stronger effect of cue validity when FP was short than when it was long (see Miniussi, Wilding, Coull, & Nobre, 1999, for converging evidence from brain potentials). From these findings, Coull et al. inferred that an unexpectedly early IS exogenously affects a sensory mechanism. This suggests that, for the invalid cues in our study, perceptual processing may have lasted longer as IS occurred earlier than specified by the cue.

Assessment of the Conditioning View

Although the major findings of this study provide fundamental support for the conditioning view of nonspecific preparation, we have come across several additional findings that were inconsistent with the specific model we proposed. First, all of the experiments showed that RT for the shortest FP was longer when the longest FP occurred on the preceding trial than when the middle FP occurred on the preceding trial. The model does not predict this result, because it does not assume that the extent to which a critical moment is bypassed makes any difference to the state of conditioning corresponding to that moment. Second, even though all the experiments showed the expected increase in RT when the imperative moment was bypassed during FP on the preceding trial, this increase was much more pronounced for the earliest critical moment than for the middle critical moment. Again, the model does not predict this result, because the distance between a critical moment and WS is not expected to modify the force of extinction when that critical moment is bypassed during FP. Third, the experiments also showed that, if anything, RT for the middle FP was longer when the predicting trial contained the same FP than when it contained a shorter FP. The model predicted the converse tendency, in view of its assumption that the state of conditioning corresponding to a critical moment increases when that moment is imperative, whereas it is left unchanged when IS occurs earlier than that critical moment.

As a first step toward a solution to these problems, it is useful to realize that these findings may have a common underlying source: the assumption that extinction and reinforcement operate on the state of conditioning corresponding to a critical moment in an all-or-none fashion. Figure 6 reflects this assumption by showing the dynamics of extinction and reinforcement during a single trial. Clearly, adjacent critical moments do not share consequences of extinction or reinforcement; that is, there is no coupling between their corresponding states of conditioning. As a result, this model has difficulties accounting for the gradual effects discussed earlier. To account for these effects, it is desirable to allow some coupling among the states of conditioning corresponding to adjacent critical moments, as is also assumed in many formal models in the literature on trace conditioning (e.g., Gallistel & Gibbon, 2000; Grossberg & Merrill, 1992; Grossberg & Schmajuk, 1989; Machado, 1997).

In particular, the learning rules of the conditioning model under present examination are very similar to those of Machado's (1997) formal model, the major difference being that the influences of extinction and reinforcement are smeared over the time scale under Machado's model. After some adjustment, Los et al. (2001) fitted Machado's model to a representative data set of FP effects, similar to the one of this study, and obtained a reasonably good fit. It is beyond the scope of this article to provide a complete account of this formal model. Instead, we indicate which modifications should be made to the original conditioning model to make it globally consistent with that of

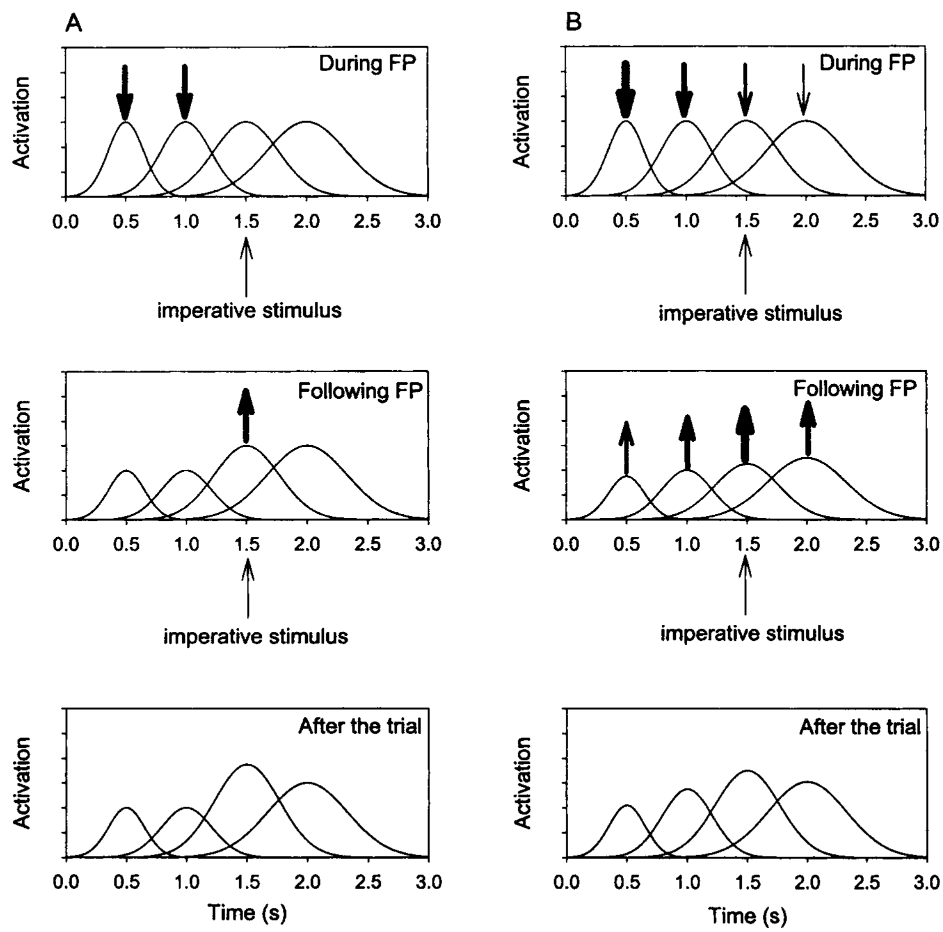

Figure 6. Within-trial dynamics of extinction and reinforcement according to the original (A) and revised (B) conditioning models. According to the original model, extinction (downward arrows) and reinforcement (upward arrows) affect each state of conditioning in an all-or-none way, thereby excluding a coupling between states of conditioning corresponding to adjacent critical moments. According to the revised model, extinction and reinforcement affect the states of conditioning more gradually across the time scale (their strength being proportional to the thickness of the arrow), resulting in a coupling between adjacent states. FP = foreperiod.

Machado.[4] Next, we indicate how the revised model accounts for the problematic findings raised earlier.

Figure 6B shows the revised conditioning model. According to this model, the state of conditioning corresponding to each critical moment is continuously subject to extinction during FP. The strength of extinction at any point in time during FP, *t*, on the state of conditioning corresponding to a critical moment, *C*, is inversely related to the distance between *t* and *C*. By integrating these extinction values across FP, the total quantity of extinction at the end of FP is obtained for each state of conditioning. Thus, even though the states of conditioning are all extinguished in parallel during FP, the revised model maintains the notion of the original model that a state of conditioning is subject to much stronger extinction when its corresponding critical moment is bypassed during FP than when it is not bypassed during FP (Figure 6B, top). Next, on the presentation of IS, reinforcement takes over. Reinforcement is maximal for the state of conditioning corresponding to the imperative moment and tapers off toward earlier and later critical moments as they are more remote from the imperative moment (Figure 6B, middle). This yields a net state of conditioning after the trial as depicted in the bottom panel of Figure 6B. Clearly, under the revised model, adjacent critical moments share to some extent the consequences of extinction and reinforcement, resulting in a coupling of their corresponding states of conditioning.

Two more specific properties emanate from Machado's (1997) model. First, for a given distance between two critical moments, the strength of coupling between their states of conditioning increases when these critical moments are more remote from WS. For instance, in an experiment with two FPs, a stronger coupling would be predicted for FPs of 1.5 s and 2.0 s than for FPs of 1.0 s and 1.5 s. Second, extinction and reinforcement are governed by a law of diminishing returns: The higher the state of conditioning, the stronger its resistance to further increase, and the lower the state of conditioning, the stronger the resistance to further decrease.[5]

The revised model accounts for the problematic findings raised earlier in the following way. First, the finding that responding at the earliest critical moment is slower when the longest FP occurred on the preceding trial than when the middle FP occurred on the preceding trial is accounted for by the gradual impact of extinction and reinforcement processes across the time scale. As the distance between two critical moments becomes larger, there is a decreased coupling between their corresponding states of conditioning. Specifically, as a critical moment is further bypassed during FP, its corresponding state of conditioning is subject to extinction during a longer time and shares less in subsequent reinforcement.

Second, the finding that RT for the middle FP is relatively unaffected by the occurrence of the longest FP on the preceding trial is accounted for by the assumption that the coupling between states of conditioning increases with their remoteness from WS. According to this assumption, there is a stronger coupling between the states of conditioning corresponding to the middle and latest critical moments than between those corresponding to the earliest and middle critical moments. That is, there is a smaller difference between the consequences of extinction and reinforcement for the states of conditioning corresponding to the middle and latest critical moments when the latest critical moment is imperative than for the states of conditioning corresponding to the earliest and middle critical moments when the middle critical moment is imperative.

Third, the finding that for the middle critical moment the gain of being repeated is less pronounced than the gain of being preceded by the earliest critical moment is accounted for by adding the assumption of the law of diminishing returns. According to this assumption, the "pulling force" of extinction and reinforcement processes is scaled by the initial state of conditioning at the start of the trial: If the state of conditioning is high, extinction will be relatively strong; if it is low, reinforcement will be relatively strong. Thus, if the state of conditioning corresponding to a critical moment is high, it may well turn out that it receives more net reinforcement (or less net extinction) when IS is presented just before that critical moment than when it is presented precisely at that critical moment. In the latter case, the additional extinction during the final phase of FP may outweigh the advantage of stronger subsequent reinforcement.

In conclusion, even though the problematic findings of this study reveal some shortcomings of the original conditioning model, they do not jeopardize the conditioning view in general. As the preceding analysis shows, it is possible to remedy these shortcomings in a way that is consistent with general assumptions deriving from formal models of trace conditioning (e.g., Grossberg & Merrill, 1992; Grossberg & Schmajuk, 1989; Machado, 1997; see Los et al., 2001, for further discussion).

Deviations From the Standard FP Design

Our focus in this article has been on a design involving a rectangular distribution of FPs of 0.5, 1.0, and 1.5 s in mixed blocks. In this section, we discuss two common deviations from this design: (a) effects of probability imbalance and (b) effects of range and mean FP.

Probability imbalance refers to any deviation from a uniform FP distribution in mixed blocks, such that some FPs occur more frequently than others. Most important in this respect is the nonaging FP distribution, for which the conditional probability of IS

[4] From our conceptual viewpoint, the model we are about to describe is perhaps not the simplest way to account for the problematic results under examination. It may be simplest to assume that the state of nonspecific preparation results from a summation of the activation values of the individual states of conditioning at any point in time. Even though the individual states of conditioning remain this way uncoupled, a coupling is realized at the level of nonspecific preparation to the extent that adjacent states of conditioning overlap. On the other hand, the model we describe shortly has the merit of giving an impression of a mathematically well-formulated model that has been successfully applied in animal learning (Machado, 1997) and FP effects in humans (Los et al., 2001).

[5] It may seem that many assumptions are needed to account for the problematic findings under examination. However, this should be attributed to the fact that we describe a mathematical model from our conceptual viewpoint (cf. Footnote 4). Starting from the mathematical model, a different set of assumptions obtains as implicated in a simple system of differential equations. Moreover, it may be recalled that the strategic model as presented in the introduction also fails to account for the problematic findings, and it may well turn out to be difficult to develop the strategic model to such a degree that it makes predictions with the same accuracy as the revised conditioning model.

presentation remains equal from the onset of WS onward. Several studies have shown that a nonaging FP distribution causes the RT–FP function to flatten considerably (e.g., Granjon, Requin, Durup, & Reynard, 1973; Näätänen, 1971). The strategic view provides a straightforward interpretation of this result: An increase in the (conditional) probability of IS presentation at a critical moment encourages participants to enhance their state of preparation for that moment. Thus, responding at the earliest critical moment is faster for nonaging FPs than for aging FPs, because of a higher probability that IS is presented at the earliest critical moment. However, the conditioning view provides an equally plausible account of these data: Raising the probability of IS presentation at a specific critical moment leads to more frequent reinforcement of the corresponding state of conditioning. Thus, responding at the earliest critical moment is faster for nonaging than for aging FPs, because this moment is relatively often repeated, causing its corresponding state of conditioning to remain high.

Along these lines of reasoning, the conditioning model also readily accounts for other effects of probability imbalance. For example, Alegria (1975) and Baumeister and Joubert (1969) used a rectangular FP distribution as well as distributions that were skewed to the left (i.e., a prevalence of long FPs) and skewed to the right (i.e., a prevalence of short FPs). Both studies revealed that mean RT for the shortest FP was shortest when the distribution was skewed to the right. It is important that, in both studies, this effect was found to be largely if not completely due to sequential effects. Thus, apart from the strategic view advocated by Alegria, a conditioning view offers a plausible account of these data.

The second deviation from the standard FP design concerns the effects of FP range and average FP. Elliot (1973) presented five FPs in mixed blocks with FP ranges of 2, 6, or 10 s and an average FP of either 6 or 12 s. He observed that for any average FP, the larger the FP range, the larger the FP effect on RT; also, for any FP range, the larger the average FP, the smaller the FP effect on RT. Taking the strategic perspective, Elliot argued that, for a given average FP, increasing the FP range makes it more difficult to maintain readiness to respond, resulting in larger FP effects on RT. In turn, for a given FP range, decreasing the average FP makes the different critical moments more discriminable and, with that, more accessible to distinct preparatory activity, resulting in larger FP effects on RT. However, these findings also naturally derive from the conditioning view. According to the revised conditioning model developed in the previous section, increasing the distance between adjacent critical moments implies a reduction of the coupling of their corresponding states of conditioning. Because of this uncoupling, increasing the FP range results in a more pronounced effect of FP on RT. The revised conditioning model also assumes that increasing the average FP results in a stronger coupling of the states of conditioning corresponding to the critical moments. Therefore, increasing the average FP reduces the effect of FP.

In conclusion, the (revised) conditioning model not only accounts for FP effects deriving from the design used in the present study. It also accounts for effects deriving from frequently encountered deviations from this design with respect to probability imbalance, FP range, and mean FP.

Conclusion

Since the early work of Woodrow (1914), not much progress has been made in specifying a mechanism underlying nonspecific preparation during the FP. We believe that the cause of this stagnation may well be the widespread idea that nonspecific preparation is necessarily an intentional process. We think that much more progress may be possible by assuming that nonspecific preparation, as reflected by FP effects, is regulated unintentionally by the learning rules of trace conditioning. In this article, we have presented evidence for the unintentional nature of nonspecific preparation by demonstrating that classical effects of FP occur even when the intention of the participant is distracted from the imperative moment.

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